affected to a lesser extent. Walking, or even standing, unsupported is impossible. He can feed himself but it is hard work. He has noticed that the character of the speech has changed, having become slow and deliberate, but there is no difficulty in articulation. The desire to urinate is imperative. He states that his memory has not suffered and that his mind is as clear as formerly. He has never had headaches or pain of any sort and he has not noticed that his strength has failed to any great extent. Vision for distance is good.

The status taken in December, 1911, showed a fairly well nourished man presenting a great incoordination of voluntary movements. To walk was impossible. Movements of the right upper extremity showed a moderate ataxia while coordinate movements of the left upper extremity were practically impossible. As it appeared to us that the type of ataxia was cerebellar in character special attention was paid to its study.

Movements démesurés. These movements were marked, in this case being evidenced by typical dysmetrie described by Thomas and Jumentié. When the patient attempted to place the finger on the tip of the nose the finger missed the mark by a wide margin, the movement being, nevertheless, well oriented and the error was of the same degree with or without the aid of the sight. In these two characteristics the ataxia was in marked contrast to the ataxia commonly seen in tabes.

Asynergia. This was present in the left lower extremity.

Catallepsie cérébelleuse. This was present in a very striking degree. In the reclining position the legs could be held elevated above the trunk quite motionless without a sign of the ataxia to be expected. Not only was there an absence of any oscillation but the members could retain this position for long periods without the usual fatigue constantly present in the normal subject.

Adiadokokenesis. This was marked in the left upper extremity. Successive pronation and supination of the left hand was impossible; in attempting this wide excursions of the forearm were made.

Pointing tests of Barany. There were no spontaneous errors in pointing. Pointing tests after turning by means of the rotary stool were not made. Among the signs of cerebellar disease was the scanning speech. This was a very prominent symptom in this case. There was, however, no difficulty in articulation. Voltaic vertigo was tested for. An interrupted current of from 14-16 m. a. was necessary to produce an inclination of the head, but it was always to the side of the positive pole.

It was not noticed that this patient had a tendency to fall particularly to one side when he lost his equilibrium nor was hypotonia present to any marked degree.

Reflexes: The radial, triceps, patellar and achilles reflexes were present on both sides and quite lively. The reflexes on the left side were found to be increased over those on the right. The plantar reflex was in flexion. The abdominal and cremasteric reflexes could not be elicited but the anal reflex was present.

The superficial sensibility was not affected and the sterognostic sense showed no impairment.

An examination of the cranial nerves showed very little. The pupils were equal in size and reacted to light, accommodation and convergence. The movements of the eyes were normal but it seemed to be an effort for the patient to look upwards and he complained of a pain in the back of the neck in attempting this movement. The fields of vision were apparently normal when tested roughly and there was no spontaneous nystagmus. The corneal reflex was perhaps a trifle diminished but distinctly present. The facial nerves were not involved. Hearing was diminished on both sides so that the ticking of a watch could only be heard in contact with the ear. There was no involvement of the 9, 10, 11 or 12 pair of cranial nerves.

On account of the history of sudden onset, the nature of the ataxia, and its prominence on the left side a diagnosis of a lesion of the left cerebellar hemisphere, vascular in origin was made.

The patient died suddenly on February 15, apparently from a stroke of apoplexy. At the autopsy when the brain was removed no tumor mass, thickening of membranes or adhesions were found, but it was noted that the left cerebellar hemisphere appeared smaller than the right and the corresponding posterior fossa of the skull was shallower than on the right side.

There was a marked arterio-sclerosis of the vessels at the base of the brain. The brain was hardened in formalin and afterwards placed in Muller's fluid. Sections showed two symmetrical areas of softening in the central white matter of both cerebellar hemispheres. The softening on the right side appeared recent. Another area of softening was found a little farther forward on the left side in external relation to the left superior peduncle. In hardening it will be seen that the inferior and superior peduncle on the left side have not taken on the brown color as on the opposite side. It would be premature to state positively before stained sections are made that these peduncles are degenerated but the gross specimen seems to indicate it. There was no evident lesion in any other portion of the brain.

In comparing the ataxia in these two cases it will be noted that the ataxia was far greater in this second case whereas the lesion was much less extensive. The question arises in this connection as to the result of slow or sudden destruction of cerebellar substance. In the case of sudden destruction as in softening it is possible that the coordinating function be permanently lost, while in the slower process the higher centers such as the cerebral hemispheres may assume this function by a process of reeducation.

Another question* is whether death by apoplexy may be caused by a lesion limited to the cerebellum. In the last case a complete autopsy was not performed so this point can not be settled in this particular case. In a case of chronic cerebro spinal meningitis observed recently, however, in which a complete autopsy was done the only lesion of sufficient importance to account for the sudden and unexpected death was a large area of softening in the roof of the 4 ventricle. This patient showed marked improvement in the meningeal symtoms prior to his death.

FACTORS IN THE PHYSIOLOGY OF BONE IN RELATION TO SURGERY.*

By ARTHUR L. FISHER, M. D., San Francisco.

My reason for presenting this paper at this time, is that it seemed to me from the discussions following Dr. Sherman's paper at the last

* Since the reading of this paper an article by Bernstein, "Kleinhirnblutung als Ursache plotzlichen Todes" (Deu. militararztl. Ztchr. No. 22, 1912. Abstracted in Deu. Med. Wochen. Dec. 12, 1912, p. 2379), dealing with this question has come to the notice of the author. A case is reported of death following a hemorrhage into one cerebellar hemisphere. Bernstein states that to his knowledge such a lesion has never been mentioned as a cause of death before, and he offers as an explanation secondary vascular disturbances in the nearby vessels of the medulla. This explanation would hardly be applicable to cases of softening. It seems to us more probable that in our cases where the lesion extended to both hemispheres the cause of death may be sought for in the loss of the co-ordinating function. In the case of cerebro-spinal meningitis mentioned above, bulbar paralysis appeared to be the direct cause of death.

* Read before the Surgical Section of the San Francisco County Medical Society, September 17th, 1912.

meeting of this section, that there was not a very definite understanding among those who are operating on bone as to what happens or may happen after the operation. To say that a nail or a screw will remain firm, or will get loose, or come out, without enumerating the surrounding conditions, does not indicate either a clear understanding of the subject or a disposition to credit the word of other men as to results observed by them. The foreign substances may remain firm or may get loose. It is a partial enumeration of the factors that influence this behavior that I shall attempt to give. .

I have nothing new, nothing original, to offer though this is a subject that lends itself most readily to experimental study. The facts I here present I have picked up here and there, frequently in articles not dealing with the subject of bone physiology primarily, but incidentally mentioned.

The teaching and study of physiology is carried out along fairly well defined lines, but the physiology of bone is one that has not attracted any considerable amount of study. A search through the *Index Medicus* and the Surgeon General's Catalogue reveals a surprisingly small number of articles on this subject. In the *Index Medicus,* bone and muscle are grouped together but bone seems to be put in the heading largely as a matter of courtesy.

Bone is living tissue, a specialized connective tissue, but I fear that it is frequently regarded as so much hard substance, wood or what not, to be cut and sawed and have nails and screws driven into it; but as it is a living tissue, and not like so much dead wood it responds to stimuli, and responds in a definite way, and it is our business to study and try to understand the character of response to each set of stimuli, to analyze them and not take things for granted.

I take it that this is to-day a real live subject, for, as one author has recently said: "Bone surgery is coming into its own again." That is, it is coming to occupy the place formerly held by the ovaries and the appendix, and that is at present occupied by the tonsils, by which I mean that hundreds of people who get the opportunity, and simply because they have the opportunity, think they are justified in operating on bone.

Bone responds to stimuli and these stimuli are both chemical and mechanical. The chemical stimuli affecting the bones in general, as, in disorders of internal secretions, acromegaly, osteomalacia, etc., or in diseases such as rachitis I do not propose to deal with here, but only with chemical stimuli affecting bone locally which will be taken up later.

Among the physical effects comes: First and foremost the general biological law that constant pressure causes atrophy, and intermittent pressure causes hypertrophy. This is a well-known biological law, and needs no special demonstration to prove it here. Let us see how it may or can apply to bone. The experiment Dr. Sherman mentioned—the nail with the constant pull on it from a rubber band—illustrates a part of this law, namely, continued pressure causes atrophy. The constant pressure of the rubber band pressing the nail up against the bone caused an atrophy of the bone ahead of the nail. If the same condition were applied to the screw in a Lane plate the same would be true, the screw would loosen.

The other part of this law, intermittent pressure causing hypertrophy, can be seen illustrated by a patient with a slowly uniting fracture of the tibia, for example, in a plaster cast; kept flat in bed, the fracture unites slowly; allowed to get up and stump about on his cast and union will take place much more rapidly. It is illustrated again by the familiar fact that the strong muscles are attached into rougher and heavier surfaces and lines on bones than are weak muscles. This is in part (not wholly) due to the greater intermittent force applied.

Weight bearing lines in bone form heaviest where the force is greatest. This was emphasized by Julius Wolf.

In the correction of bowlegs, e. g., by pressure or osteotomy, the whole bone gradually straightens out. As weight is applied to the more concave part of the bone, the outer convex portion of the bone is absorbed, gradually disappearing entirely, and more and more bone is deposited in the weight bearing lines. It is this law also that is made use of in the correction of scoliosis.

That the local temperature has an effect is shown by the following extreme experiment reported by Rippert in the *Deut. Med. Woch.* 1909. He found that when the legs of animals were made bloodless, and then put in a freezing mixture for ten minutes, that the animals regained the use of the legs after a few days, but that the bone became necrotic; the cartilage, the periosteum, and the medulla remain living; the bone does not sequestrate but becomes covered with a new layer of thin bone, formed from the periosteum and the endosteum. This is, of course, an extreme procedure, but how frequently do we see bone made bloodless by an Esmarch bandage, exposed to lowered temperature for more than ten minutes. A careful series of experiments along this line would be a very valuable addition to our knowledge.

Also, Schepelman experimenting with hot air in the treatment of fracture in rabbits found that the hot air inhibited, to a certain extent, the formation of large callus.

The introduction of foreign substances into the tissues in general and also directly into bone, has, of course, been the subject of innumerable clinical experiments as well as some direct laboratory experiments. Lange of Munich, and his assistants Von Baeyer and Engelhardt, have studied foreign substances, particularly metals, and find that the chemical character makes a considerable difference, certain metals causing aseptic suppuration; for example, copper, and some metals being well borne; such as tin. They found also that electrical conditions must be taken into consideration, and if two metals are used aseptic pus is deposited around the negative electrode, for

example, when copper and zinc are used together the pus collects only about the zinc even though the copper alone always attracts the leukocytes. Thus it may be of considerable importance in the use of Lane plates, to consider the metals of which the plates and the screws are made.

Meisenbach of Buffalo, tried to demonstrate the influence of certain chemicals on the growth of bone. He injected various substances into the epiphysis of bone of young rabbits, and then studied them with X-Ray and histologically. The substances used were sterile water, sterile graphite pegs, staphylococcus vaccine, pure tincture of iodine, pure carbolic acid, pure alcohol, pure formalin, and two per cent formalin. With sterile water the results were negative, with sterile graphite pegs the results were on the whole negative, but one showed some thickening of the cortex and it is noticeable that the peg changed its position. The hole made by the canula always closed. In rabbits in which the graphite peg and staphylococcus vaccine were used together, six out of seven showed a thickening of the perichondral and endochondral bone in the diaphyseal region. No change was noticed in the epiphyseal line. Pure tincture of iodine showed no effect. The pure carbolic acid showed only slightly increased vascularity. Alcohol showed no change. With pure formalin all the rabbits showed a thickening of the cortex, irregularity of the epiphyseal growth, exuberant growth of the diaphysis, and a general widening and thickening of both epiphyseal and diaphysis. Two per cent. formalin showed similar changes, but not as extensive. His conclusions are as follows:

Bone can be stimulated to growth by chemical, mechanical and biochemical means. Mechanical stimulation chiefly affects perichondral bone formation, whereas chemical stimulation effects the epiphyseal line directly, causing proliferation of the cartilage cells and increases zones of provisional calcification and calcified matrix together with osteogenic tissue derived from the perichondrium Mechanical stimulation is slow, whereas chemical stimulation is rapid. The combination of chemical and mechanical stimulation increases both perichondral and endochondral bone formation. Retardation of growth may occur if the zone of provisional calcification is destroyed, or if this zone is invaded by excessive blood clot or by destructive process.

Of all the substances used formalin gave best results on account of its antiseptic properties, and its affinity for protoplasm. Formalin injected upon epiphyseal line, becomes an insoluble compound and therefore affects the epiphyseal line, both mechanically and chemically with the distinct local rather than a systemic tendency. It causes formation of osteogenic tissue by influencing the zone of provisional calcified matrix, and by increasing the number of osteoblasts from the perichondrium.

This work of Meisenbach taken in conjunction with some done by Parsons and reported in the *Jour. of Anat. and Phys.* 1904, may throw some light on the formation of epiphyses. Parsons first points out that the epiphyses are not essential for (though they may be useful in) bone growth, as there are none in birds, except in one species. He divided all epiphyses into traction and pressure epiphyses—the traction epiphyses are those into which the strong muscles are inserted; and the pressure those in which the weight is borne from above. He also shows that it is the largest cartilagenous ends of bone that show epiphyses first, and that these occur in the center of the cartilagenous ball. At this point the pressure is greatest; now it is not only the pressure, as this would not explain why they occur in the largest first, but he believes that it may be that the centers degenerate first, and it is the blood vessels that grow in, in response to this degeneration, that carry the bone-forming cells with them. This may also be true in Meisenbach's experiments; that his chemicals cause slight necrosis, and in answer to this, the blood vessels grow in, carrying along the bone forming cells.

In the treatment of ununited fractures there is much more to consider than the mere approximation. Approximation does not mean union. Non-union occurs in 1 to 2 per cent. of all fractures, and it is only a small percentage of this 1 to 2 per cent. that does not unite on account of mechanical obstacles, interposed soft parts or a distance between the fragments.

Metal sutures of all sorts probably increase the normal softening that occurs about a fracture, and then the suture fails to hold. Also if the periosteum is ripped up it adds to the softening process, delaying union. If bones are cut they should not be sawed off too neatly, but rather leave slightly roughened edges to allow a certain amount of blood clot which will cause hyperemia, as in ununited fractures increased blood supply seems to help.

In all cases of operative procedure on bone, and particularly fractures that are operated upon, let us try to be a little more accurate in what we are doing and let us try to study the surrounding conditions so that we can arrive at some definite conclusion as to the value of given procedure.

Discussion.

Dr. Raymond Russ (presiding): Dr. Fisher is certainly to be congratulated upon this paper. If there is one subject that provokes discussion, it is the use of the Lane plates in fractures. Each man takes his own experience, and bases his conclusions on them. I have thought for some time that a study of the physiology of bone in relation to these plates would be most interesting. I think the Society should thank Dr. Fisher for his paper.

Dr. Samuel Hunkin: I am sure that I do not agree with Dr. Fisher in some of his statements, regardless of the authorities which he has cited. If I had a fracture, the nearer it could be gotten into apposition, and the more true it could be cut, the better I should be pleased, and less callous and more rapid union I should expect. I am not disagreeing with the experiments recited by Dr. Fisher, but with the conclusions he has drawn from them. I do not believe, for instance, a fracture of the tibia under ordinary circumstances would unite quicker if a fellow walked around on it than if it was kept quietly and securely in

a splint. When union is delayed, however, then we know that weight bearing stimulates the production of callous and favors union, but not a neat repair as we like best. Dr. Fisher also made the statement that bone is easily rendered bloodless by Esmarch bandage for a long time. Now it is the hardest thing in the world in my experience to make bone bloodless unless you control the artery above the nearest proximal joint, and it does not get so very bloodless even then.

I do not see, under the reasoning given, how Dr. Fisher accounts for the increase of growth which sometimes follows tuberculosis, especially in the lower end of the femur. Personally also I believe that the epiphysis is a mighty mportant thing in the promotion of growth, much more so that Dr. Fisher would have us believe. Apparently in children, it is essential to growth, and if anything interferes with it the growth comes mighty close to stopping. I do not know how it is in birds, but if it is as Dr. Fisher says, I would not care to have my child treated like a bird could be treated for injuries around an epiphysis.

Dr. A. L. Fisher, closing discussion: In reply to Dr. Hunkin, I am not decrying accurate approximation—what I said was that union does not depend upon approximation.

With regard to the bone being bloodless, I know it is difficult to make it absolutely bloodless; but a relative shutting off of the blood supply is not difficult to accomplish.

About tuberculosis: I think the answer to that (this is purely my own idea) is that the chemical products formed by the tubercle bacilli in and about the joints stimulate the bone to growth. Whether the stimulation of cells is increased by the chemical products of the tubercle bacilli, or by the necrosis of the bone and the new growth in response to the necrosis, is more than I attempt to say. The experiments of Meisenbach are suggestive in this way. He injects substances and the bone becomes thickened. The growth is irregular, but he gets a great deposit about the point of injection.

THE ARNETH BLOOD COUNT IN PULMONARY TUBERCULOSIS— A REPORT OF 80 CASES.

By R. S. CUMMINGS, M. D., Los Angeles.

In 1896 A. M. Holmes[1] noted there was a morphological change in the neutrophile polynuclear leukocytes in tuberculosis. Arneth,[2] however, in 1904 was the first to note a definite relation between the state of the nucleus and the patient's condition.

He divided the neutrophile polynuclears into five classes, according to the number of neclei each contained. Class I containing one nucleus, class II containing two nuclei, class III containing three nuclei, class IV containing four nuclei and class V containing five nuclei. He found in the normal person the following average polynuclear neutrophile pictures: 5% of cells fell in class I, 35% fell in class II, 41% fell in class III, 17% in class IV, and 2% in class V. In tuberculous patients, however, the count was frequently as follows: 12% in class I, 48% in class II, 30% in class III, 1% in class IV, while Class V contained none. This shows a marked increase in the cells with one and two nuclei with a corresponding decrease in those with four and five.

Arneth believed that tuberculosis could be diagnosed by an increase of the cells containing one and two nuclei, a condition which he termed shifting to the left.

In this country Klebs[3] in 1906 confirmed Arneth's findings regarding a shifting to the left in tuberculosis.

In 1908 Bushnell and Treuholtz[4] reported a series of observations both upon normal and tuberculous persons. They established what they termed an index by adding classes one and two and half of class three of each 100 cells, which facilitated a comparison of observations. They found their index for normal persons to be 67, i. e., 67% of the cells fell in classes I, II and one-half of class III, while in the tuberculous the index would rise to a height of 85-90, showing a marked shifting to the left.

In 1909 Minor and Ringer[5] reported their findings which served to confirm Arneth's contentions. They were very enthusiastic over the result of this method in assisting to more correctly prognosticate the condition of the patient, which enthusiasm was somewhat moderated by further work as recently reported by Ringer.[6]

Later Reed-Lewis[7] and Miller and Reed[8] confirmed the observations of Arneth upon cases observed for many months. From counts upon thirty apparently normal persons whose average index was found to be 48, they concluded that anything from 45-55 could be considered normal.

They also found a marked deviation or shifting to the left in tuberculous patients, the worse the condition of the patient the greater the deviation.

Cohen and Strichler,[9] reporting observations made upon tuberculous patients, found that as a patient improved there was a shifting to the left in place of a shifting to the right as reported by other observers, i. e., they found an increase of class IV and V and a diminution of class I and II.

It is impossible to understand why their results were so adverse to those of other writers; one criticism, however, is that no Arneth counts upon normal persons were reported, thus establishing their normal index.

Kagan[10] also concluded from his observations that the Arneth count was unreliable.

Williams'[11] results average about the same as other observers with the exception that he found great variation in his normal counts.

The results of Miller, Lupton and Brown[12] were similar to those of the majority of observers. They concluded that the Arneth count could not be used as a guide to dosage of tuberculin.

Briggs'[13] observations tend to confirm Arneth's contentions. His normal index was 58, he having made 30 counts upon 17 apparently healthy persons. He observed a shifting to the left in active tuberculosis.

A vast deal of discussion and work has been produced among German observers, a rather complete bibliography of which is given by Miller and Reed and Schilling-Torgau.[14]

In all of our examinations 200 cells were counted, 100 on each of two coverslip smears. Blood was taken from the ear and very thin smears were made, great care being exercised to avoid rupturing